human physiology in particular was largely nonsense.

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Colquhoun and Foreman (March 1997 *JRSM*, p 178) are quite right to state that medical students should be taught to distinguish principles based on evidence from those based on false premises. Homoeopathy is an example of alternative medicine based on a false premise often rendered in Latin as *similia similibus curantur*—like cures like.

Scott and McCourt wrote a book about homoeopathy, which they say 'is free from dogma and prejudice'[1]. Homoeopathy, they continue, 'simply means treating diseases with remedies similar to the disease in question when taken by healthy people'. This 'principle' or law of similarity is a dogma without any pharmacological foundation. The ancient origins of a therapy are often cited as a justification for its use. Hippocrates and Paracelsus are supposed by some to have accepted the similarity law as true. It may, however, have had its origin in the magical rites of sorcerers described by Fraser as homoeopathic magic or the law of similarity, one of the two types of sympathetic magic or the law of sympathy.

Hart and colleagues (February 1997 *JRSM*, pp 72–87) conducted a controlled trial of arnica diluted sixty times, for pain and infection after total abdominal hysterectomy. At that dilution the administered liquid could not have contained a molecule of arnica, as Dr Youngson points out in his well-argued letter (April 1997 *JRSM*, pp 239–240). If alternative medicine therapies are based on false principles do we need controlled trials to prove their lack of worth?

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The logic followed by Colquhoun and Foreman, in concluding that our paper illustrates why complementary medicine should *not* be introduced into the medical curriculum, is obscure. We showed that medical students are interested in complementary medicine, and that many patients they will encounter in their medical careers will be receiving such treatment. We might have added that, according to a study commissioned by the Department of Health, 40% of British general practices provide access to complementary therapies[1], or that in other parts of Europe as many as 95% of general practitioners practise some form of complementary medicine[2]. Complementary medicine is a reality of contemporary medical practice, and students are interested in it. This seems to us to constitute a strong case for including it in the curriculum.

Some of the confusion seems to arise because Colquhoun and Foreman confound the educational and evidence issues. Our article explicitly addressed the former, although clinical effectiveness is, of course, very important. However, we are mindful of the General Medical Council's view that the medical curriculum should not be overloaded with facts, so our suggestions for course content emphasize critical discussion rather than learning of facts. Any implication that there is no clinical evidence for the effectiveness of complementary therapies, or that we wish to avoid the question of clinical effectiveness, is unfounded. The Royal London Homoeopathic Hospital NHS Trust has recently published *The Evidence Base of Complementary Medicine*, summarizing the clinical trial evidence on homoeopathy, acupuncture, manipulative therapies and nutritional medicine. A copy of this document will be sent to any reader of the *JRSM*, free of charge, on request.

Equally unfounded is Professor Colquhoun's claim to have discovered an 'elementary statistical error' in a clinical trial in which one of us (PF) was the principal investigator. This claim was refuted soon after he made it, in a letter to *The Lancet* that he does not cite[3]. The facts are as follows: after the publication of this randomized, placebo-controlled trial in the *British Medical Journal*[4], Professor Colquhoun requested the raw data, which PF supplied. Professor Colquhoun reanalysed the data using a non-standard statistical test (the randomization test) with a program written by himself, and claimed that, although the effect on the main outcome parameter was statistically significant, there was a significant treatment/period interaction. The original authors had the data analysed again with standard methods and software and again found no evidence of such an interaction. The original case record forms from this trial have been retained; PF will supply the same data, on disk, as were supplied to Professor Colquhoun, to any reader of the *JRSM* who wishes to draw his or her own conclusions.

Marvin McMillen of Yale Medical School has remarked that homoeopathy has 'often been discussed by academic physicians and scientists in terms more reminiscent of the religious wars of the 16th century than respectful academic discourse'. By introducing allegations of religious zealotry, Colquhoun and Foreman regrettably bear out this observation. We believe that the interests of students and practitioners of medicine, and above all of patients, would be better served by education and debate around the evidence.

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I hope that the letter from Dr Youngson will not dissuade your journal from examining aspects of alternative medicine, especially homoeopathy. The path of the

undoubted advances in the biosciences since 1810 has not been without considerable disagreement along the way and would not have emerged if a line had been drawn at any stage to restrict discussion. It is easy to lose sight of the fact that every research paper is participation in an argument, now that the forms and methodology of research are so widely accepted.

In the light of current scientific knowledge there is no explanation for the effect of the extreme dilutions of actives used in homoeopathy and yet it would be churlish to propose that practitioners of homoeopathy do not achieve results. If these results are entirely due to placebo effects, it should be possible to demonstrate this in terms that conform to the most stringent scientific criteria. Alternatively, if there is no therapeutic effect it should also be possible to demonstrate why so many people avail themselves of this form of treatment.

However, the paper by Hart *et al.* is not examining homoeopathy but the administration of arnica C30. It is a natural result of reasoning by those trained in reductionist techniques to abstract an 'active' from a herbal or homoeopathic armamentarium and test that, rather than the total system in its holistic form. The study would have gained greater credence if a group had been included to undergo treatment by competent homoeopathic practitioners for some time before undergoing surgery. For such a study it would also be necessary to cover the possibility that surgery was no longer indicated.

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Intraluminal stenting in the management of adhesional intestinal obstruction

Mr DeFriend and colleagues (March 1997 *JRSM*, pp 132–5) provide a welcome reminder that recurrent small-bowel obstruction due to multiple adhesions remains a serious problem for a small group of patients. As they remark, there is little evidence to support adding intubation to adhesiolysis at the first laparotomy for adhesive obstruction, or for its use in obstruction due to a single band. What they do not comment on is the principal indication for use of a long intraluminal stent, which is to offer the chance of long-term relief to those who suffer repeated episodes of adhesive obstruction.

I have described the plight of the patient who first led me to use intubation in 1972[1], and I continue to believe that it was the addition of intubation to complete adhesiolysis which enabled us to terminate her long sequence of laparotomies, to close her persistent enterocutaneous fistula, and for her then to experience (at the last check) 13 years of good health. It seems reasonable to believe that intubation played a part, at least, in the fact that the 15 patients of DeFriend and colleagues (who had twenty-three previous adhesiolyses) remained free of trouble for 2–10 years. In the other 16 patients we reported in 1985, who had already had twenty-seven operations, 9 remained free of obstruction for more than 7 years, and another 4 for over 4 years[2].

Sometimes the facts have to speak for themselves, and those who required a control series, to evaluate the role of intubation, overlook the fact that all these patients have been treated by the control operation—i.e. adhesiolysis. In each case this operation has failed them, in some cases two, three or four times, and this record was only ended after careful separation of every adhesion, the elimination of short circuits, closure of fistulae and intubation.

In their summary, DeFriend and colleagues give prominence to 'the high rate of complications', but it must be questioned whether this should act as too much of a deterrent in treating a group who have already experienced their full share of the complications of small-bowel adhesions. In fact, major complications are unusual. Weigelt and colleagues in 160 intubations did not see a case of enterocutaneous fistula at the site of the jejunostomy; neither did we in the course of 126 intubations[2]. Reports of intussusception are within single figures. In 91 quoted examples of intubation in patients who had at least one previous operation to relieve adhesive obstruction, there was only one instance of later recurrence of obstruction which required operative relief[2].

These facts should encourage rather than deter the use of intubation in the treatment of this small and deserving group, because it does hold the hope of long-term relief from recurrent obstruction.

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Evolutionary psychiatry

Discussing Stevens and Price's book *Evolutionary Psychiatry*, Professor McManus (March 1997 *JRSM*, pp 174–5) dismisses group selection by claiming that 'hardly a serious worker currently believes in it'. This is not so. According to Morell[1], a growing number of researchers say that it deserves a fresh hearing. McManus also states that 'There are only individuals; but sometimes self-interest and the interests of the group can be made to coincide'. Acts such as sharing of resources with non-kin, caring and consolation toward non-kin, and attempts to reconcile non-kin have directly nothing to do with the self-interest of any chimpanzee whatever[2–4]. Those acts can only make sense in the context of an evolutionary strategy for primarily promoting group welfare. Since chimpanzees and humans share 98% of their genetic code, we should consider the behaviour of our evolutionarily closest relatives as being relevant to ours[4].

The mere fact that evolution has fostered socially useful behaviour constitutes the best evidence that groups displaying such behaviour have an advantage over groups that do not display it. This is particularly evident in Italy, where those who live in the southern regions are notoriously more individualist and less likely to denounce socially devastating criminals than those who live in the northern regions. This heavily contributes to the endemic problems of the South of Italy. By contrast, low individualism, high social cohesion, and profound concern for community well-being play a substantial part in maintaining the prosperity of both Germany and Japan.

While today even socially defective groups can survive thanks to either national or international solidarity, such groups of the ancient past were more likely than socially perfect groups to be overcome by the harshly savage environment. No wonder that 'an increasing number of biologists chafe against the idea that individual